the precise significance of these words, as lately determined by the Council of the Society.

Two courses appeared to be open: either to submit proofs to the authors of the abstracts of their papers sent to the Society, if any substantial (*i.e.* more than typographical) alteration had been made; in which case, the authors themselves would naturally bear the responsibility of their statements; or to throw the whole responsibility on the Editors, leaving them to make any excisions or alterations they may choose in the abstracts sent to them; or indeed, if they so think fit, entirely to rewrite them. The Council, in order to secure rapid publication, have chosen the latter alternative; and it should be understood that the abstracts are now "official"—*i.e.* the responsibility for all statements put forth rests solely on the Editors of the *Proceedings*. WILLIAM RAMSAY.

University College, London, W.C., January 14.

Philosophy and Natural Science.

WHILST feeling obliged to your reviewer's appreciation of my essay (p. 220), I am bound to rectify some very glaring discrepancies.

(1) As plainly stated in my preface, my essay has *not* obtained the Philosophical Society's prize, but only an "honourable recognition," and two fifths of the prize sum.

(2) Eighth line from bottom (p. 220), for "physical," read "*psychical*," as said in my paper (p. 30).

(3) Your reviewer makes me say: "Physical development is not the cause, but the effect of psychical development"; whereas, I have expressedly *combated* this view of Wundt's (p. 32).

(4) Neither did I say: "The modifications in the brain and nervous system throughout the animal kingdom are intelligible as resulting from psychical causes . . ." but only (p. 32) that *in many cases* the beginnings of modifications are intelligible from the psychical side—*e.g.* the modifications of many organs—resulting from sexual selection.

(5) Lastly, far from saying that the high mental position of man, on the one hand, and of ants on the other, "is independent of the structure of the nervous system," my sentence (p. 34) is: "Here, where the organic substratum (*i.e.* the brain) in both types differs even in its principal morphological features, it is most evident how occult are the processes which constitute the proper material side of psychical phenomena."

Freiburg, Badenia, January 5. DAVID WETTERHAN.

(1) THE facts are that the Philosophical Society of Berlin offered a prize of 1000 marks for an essay on "The relation of philosophy to the empirical knowledge of nature."

The essay reviewed, only obtained 400 marks of this prize, and an honourable mention. In a hasty glance at the preface I overlooked the words "ein Antheil von vierhundert Mark," which occur in the next line to "der als Preis ausgesetzen Summe," which caught my eye.

(2) This is evidently a slip of the pen, which I regret was overlooked in proof.

(3) In my notes, jotted down as I read the pamphlet, I put Wundt's words in quotation marks, intending to point out Mr. Wetterhan's opposition thereto; but in writing the review, I unfortunately omitted the commas, and, I regret, entirely misrepresented the author's views. Perhaps I may quote from p. 32 of the pamphlet: "Man durfte Wundt's Satz, 'dass die physische Entwicklung nicht die Ursache, sondern vielmehr die Wirkung der psychischen Entwicklung ist,' zu weitgehend, und auch in seinen Konsequenz bedenklich finden." We are then referred to page 46, where we read: "Der Ausführung dagegen, welche Wundt (s.o. p. 32) jenem Prinzipe gegeben hat, vermag ich kaum eher beizustimmen, als der verwandten Ideen Schopenhauer's."

(4) It appears to me that the passage will bear the construction which I put upon it; though perhaps "throughout" the animal series is too inclusive as a rendering of "der Tierreihen."

(5) The author had been discussing the similarity of habits and instincts in ants and termites, and then remarks that there is a distinct agreement in the mental functions ("von geistigen, ja gemütlichen Funktionen") of bees with those of the higher animals. He refers to Darwin's opinion that the small brain of a bee is a more wonderful thing than the brain of a Man: and I think I was entitled to make the obviously true remark that this "mental development is independent of the structure of the nervous system." I was not quoting Mr. Wetterhan's words, but giving the general sense of the passage.

In conclusion, I must express my regret that the condensation of some of the author's remarks should have resulted in a confused expression of his views. THE REVIEWER.

SOME EARLY TERRESTRIAL MAGNETIC DISCOVERIES PERTAINING TO ENGLAND.

IT should be a source of considerable pride to British men of science that so many of the discoveries in terrestrial magnetism have been made in England. And yet, owing to the absence of a complete and carefully written history of the development of this science, probably few could enumerate all the achievements in this subject by Englishmen.

In February 1893 the writer had the good fortune to light upon a book,[1] by Will Whiston, containing matter pertaining to the terrestrial magnetism of England, which appears to have been entirely overlooked by prominent terrestrial magneticians. Owing to pressure of work, this interesting book, of which a copy was found in the Royal Library of Berlin, could not be subjected to a critical examination until the early part of 1894, when the writer called the attention of prominent Berlin investigators, such as Prof. Hellmann and Dr. Eschenhagen, to it.[2] In the meantime, Dr. W. Felgentraeger, Assistant at the Göttingen Magnetic Observatory, made an independent discovery of Whiston's book, and carefully worked up part of the material contained therein.[3] The writer has since found time to complete his examination of Whiston's contribution, and has embodied his results in a paper[4] presented by Prof. Cleveland Abbe before the Philosophical Society of Washington on November 10, 1894. In the following these results will be briefly sketched.

As will appear from the title of Whiston's work, the chief object was the exposition of a method for determining the longitude and latitude by means of the magnetic dip-needle, *i.e.* by means of the angle which a magnetic needle mounted on a horizontal axis, when placed in the vertical plane of passing through the magnetic meridian, makes with the plane of the horizon. It will be recalled that at that time great prizes had been offered by the English Parliament for an easy and trustworthy method of determining longitude at sea. From the very birth of terrestrial magnetism we find methods proposed for determining longitude by means of magnetic observations, and, like the problem of perpetual motion, these magnetic methods were revived every once in a while until the beginning of the nineteenth century. Owing to the irregular distribution of magnetism within the earth's surface, and on account of the many fluctuations terrestrial magnetism is subject to, these magnetic attempts to determine geographical position have been doomed to failure. They, nevertheless, have done much to promote the science of terrestrial magnetism. A striking instance of this is the book of Whiston's. The prime object of the book has failed of its purpose,[5] yet the incidental discoveries, the importance of which the author himself did not fully appreciate, may perpetuate the name of Whiston for ever. It is most remarkable that his contributions have been, apparently, entirely overlooked.

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[1] "The Longitude and Latitude found by the Inclinatory or Dipping Needle; wherein the Laws of Magnetism are also discover'd. To which is prefix'd an Historical Preface; and to which is subjoin'd Mr. Robert Norman's New Attractive, or Account of the first Invention of the Dipping Needle." By Will Whiston, M.A., sometime Professor of Mathematicks in the University of Cambridge. (London, 1721. 8vo, xxviii. 115, iv. and 43 pp. 2 charts and 3 cuts.)

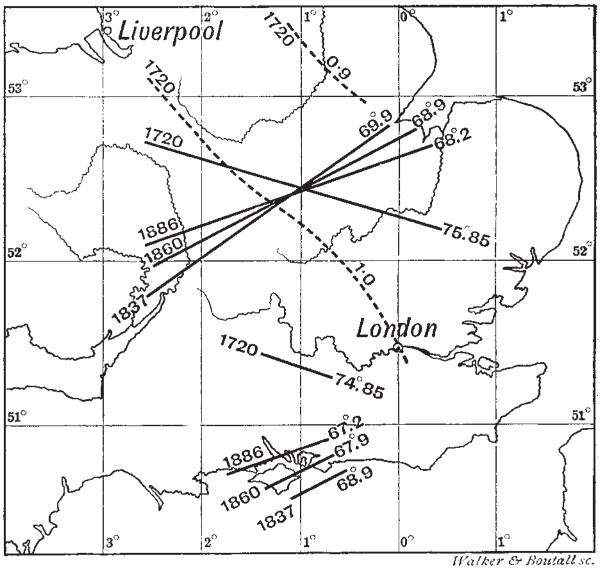
[2] See remarks in *Physical Review*, vol. ii. No. 1, p. 72.

[3] "Die Isoklinenkarte von Whiston und die säkulare Aenderung der magnetischen Inklination im östlichen England." Von W. Felgentraeger. Reprint from *Nachrichten der k. Gesell. der Wiss. zu Göttingen Math. Phys. Klasse*, 1894. No. 2. 8vo, 12 pp.

[4] Entitled "The Earliest Isoclinics and Observations of Magnetic Force." (*Bull. Phil. Soc., Wash.*, vol. xii. pp. 397-410.)

Whiston, who, as stated in the title of his book, was at one time Professor of Mathematics at Cambridge, being Sir Isaac Newton's successor, was banished not long after assuming the chair, on account of heresy—he was a Unitarian. He was led to pursue the longitude problem magnetically through Halley's famous Isogonic Chart of 1700,[6] which came under his notice. As is well known, this chart of Halley's, giving the lines of equal magnetic declination, *i.e.* these lines on the earth's surface connecting all the places at which a magnetic needle swung horizontally has the same bearing, is the earliest published chart of its kind. In consequence, these lines have likewise been termed the "Halleyan Lines." Since then Halley's method has been effectually applied to the representation of other terrestrial phenomena, *e.g.* distribution of temperature ("Humboldt's Isotherms," 1817). Wilcke is credited as first applying Halley's method to the representation of the distribution of the magnetic *inclination*, and Wilcke's isoclinics are therefore referred to occasionally as the "Wilckean Lines." Wilcke published his chart, covering the greater portion of the earth, in 1768.[7] It appears, however, that the credit of first drawing the isoclinics should be accorded to Whiston. Wilcke nowhere states in the article cited that he for the first time has drawn the lines of equal magnetic inclination, and it is, moreover, reasonable to suppose, by his reference to Whiston's book, that he was familiar with its contents.

Whiston was led to drawing the isoclinics upon finding that the "Halleyan Lines," through "the Quickness of the Mutation of those Lines and their different Position in the rest of the World," could not be satisfactorily used for the determination of longitude. He therefore began to consider the "lines of equal dip,"[8] thinking they would answer his purpose better. To this end he collected all observations of dip made up to his time, and with their assistance drew, as far as then possible, the "lines of equal dip" upon Mr. Molyneux's terrestrial globe. Furthermore, to practically test his method, he made dip observations himself in 1719 and 1720 in various portions of England, and with their aid drew and published the *first isoclinics*, to be sure for only a small portion of the earth, viz. for southern England and north-western France. These isoclinics were laid down on two small charts (11·4 × 18·2 cm.), and are given opposite p. xxviii. of his book. The first chart is the result of dip observations made in 1719 with a needle 12 inches long; the second[9] is based upon more numerous observations made in 1720 with a $47\frac{1}{2}$-inch needle. The results with the two needles differ, on the average, by about $1\frac{1}{2}^\circ$, the long needle giving the larger value. To counteract the error due to flexure of the long needle, Whiston placed a small "Poise of brass circular Wire," which required shifting to and fro according to dip, on the north end of the needle. Whiston believed that the longer the needle, the better the result if the needle be poised as stated. It is needless to say that experience has not borne him out in this respect. Owing to the large discrepancy between the results of the two needles, it might appear, then, that but little value can be attached to these Whistonian isoclinics. The writer finds, however, in his paper that the mean of the two results can doubtless be accepted as being within 1° of the truth. Moreover, while the *absolute* value may be impaired to the extent mentioned, the *relative* value remains intact, as the effect of the constant instrumental error would be almost entirely eliminated when considering the *relative* distribution of the dip over England. It is this latter fact that may give a value to these early isoclinics. Thus in 1720, according to Whiston, the isoclinics over England ran approximately from west-north-west to east-south-east, while to-day they go roughly from west-south-west to east-north-east. In the accompanying sketch, the mean isoclinics are shown by full lines for the epochs 1720, 1837, 1860, and 1886, as drawn by Dr. Felgentraeger in the paper cited.[3] It will be seen that between 1720 to about 1837 they shifted from year to year, *anti-clockwise;* they are now moving *clockwise.* Sabine,[10] I believe, was the first to call attention to this reversal of motion. It is hoped that this matter will receive further investigation.



Walker & Boutall sc.

Whiston comes in, however, for a still greater share in the early terrestrial magnetic discoveries. He invents, namely, a new and indirect method of determining the dip—the vibration method. He swings a magnetic needle horizontally, and determines the time of one horizontal vibration. He then swings the *same* needle mounted as a dip-needle, and again determines the time of one vibration. From the two times of vibration it is a simple matter to compute the prevailing dip. Whiston proposes this method as an approximate check upon the direct method where the angle of dip is measured at once. For example, Whiston found at London the time of one horizontal vibration of his long needle 120° from the magnetic meridian to be $60\frac{1}{4}$ seconds, which reduced to the magnetic meridian gives 42·6 seconds. The time of one vibration of the same needle, mounted as dip-needle, was found to be 22 seconds. If F is the total magnetic force, we then have the following relation:

$$F : F \cos \text{Dip} = (42{\cdot}6)^2 : (22)^2, \text{ or Dip} = 74^\circ\ 32'.$$

Now, the mean of the results of the direct measurements with the 12-inch needle ($73\frac{3}{4}^\circ$), and of the $47\frac{1}{2}$-inch needle ($75\frac{1}{6}^\circ$), is 74° 28′. Hence the agreement is quite satisfactory. We can probably say that the magnetic dip in London in 1720·0 was 74·5° ± 0·5°.

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[5] When Graham discovered, a few years after the publication of Whiston's book, that terrestrial magnetism is subject to a daily variation, Whiston perceived the inutility of his method. See "Memoirs of the Life and Writings of Mr. William Whiston. Written by himself." (London, 1749, vol. i. p 297.)

[6] Published in London in 1701, and reproduced photolithographically in Greenwich "Observations," 1869. To be soon republished in "Neudrucke von Schriften und Karten über Meteorologie und Erdmagnetismus." Edited by Prof. Dr. G. Hellmann. (Berlin, A. Ascher and Co.)

[7] "Sv. Vetensk. Akad. Handl.," 1768, p. 193.

[8] Whiston even calls these lines also "Magnetick Parallels."

[9] Reproduced in facsimile in Dr. Felgentraeger's paper (see Ref. 3). Both charts are to be given, also, in facsimile, in Hellmann's "Neudrucke," No. 4 (see Ref. 6).

[10] *Proc. Roy. Soc.*, vol. xi. p. 144: "The angle of intersection of the meridian and isoclinics has been diminishing up to about 1840, when a reversal took place, and the angle is now increasing."

The invention of this new method of determining dip, led to still more important results. In making the dip observations with the long needle in 1720, for the purpose of his second chart, he at the same time observed the time of one horizontal vibration of the same needle, with the express purpose of determining the distribution of the force. These vibration-times are tabulated on his second chart.[11] From them the distribution of relative intensity can be determined. *These observations of Whiston's are undoubtedly the earliest relative terrestrial magnetic force observations ever made.* It is usually believed that the earliest relative intensity observations are the defective ones of Mallet (1769), or the more successful ones, but lost in shipwreck, of Lamanon (1785–87). The absolute value of these Whistonian intensity observations can, of course, not be checked; however, the relative value admits of some control. Thus the writer in his paper has reduced the observations, taking the value of the horizontal force at London as unity, and, with the aid of the reduced values, has roughly sketched the isodynamics (the broken lines in the figure) as prevailing over southern England in 1720. It will be seen that these lines of equal magnetic horizontal force have the same general trend as the isoclinics, as, indeed, they roughly should. Again, taking two stations (London and Saltfleet), which are practically in the same meridian and are distant from each other 2° in latitude, through which, consequently, the same isodynamic would not be likely to pass, it is found that the difference of the vibration-times—$60\frac{1}{4}$ and 66 seconds respectively, is in the right sense, *i.e.*, since the force varies inversely as the squares of the vibration-times, *the more northerly station, Saltfleet, gives the smaller horizontal force*, as it should. To be sure these early intensity observations are affected with a large probable error; they may, however, not deserve to be assigned to utter oblivion.

Upon the presentation of the writer's paper before the Washington Philosophical Society, Prof. Abbe became interested in the matter, and kindly called the author's attention to a later book[12] of Whiston's, a copy of which was likewise found in the Royal Library of Berlin. Whiston, in this book, gives an account of dip-observations made in various portions of the earth, with the aid of most liberal means furnished by King George and others, for the purpose of testing his magnetic method of determining geographical position. He sent "four several Dipping-Needles to Sea," and "with proper Instructions to the Masters of the Vessels" to observe the dip with both methods (direct and indirect), "to discover the State of Magnetism in the several parts of the Globe." Thus Captain James Jolly set out in July, 1722, for Archangel with one of Whiston's dipping-needles. Owing to a defect of the instrument he could observe only horizontal vibrations. Whiston says (p. 84), "he made me twenty-eight very good Horizontal Observations from the Latitude of 65 quite to Archangel." . . . "In this Space the Needle altered its Velocity very greatly, as I expected it would: And 5 Vibrations which at first were perform'd in about 280″, beyond the North Cape, came to 250″; till towards Archangel it gradually returned to about 177″." The first figure, 280″, is probably a misprint, and should be 180″. *These observations are the first to show the truth of the law that horizontal intensity decreases in approaching the magnetic pole.* Humboldt has credited Lamanon (1785–87) with the discovery of this law; it was not, however, firmly established until Humboldt's observations of 1798–1803. Furthermore, Captain Othniel Beal set out about the same time as Captain Jolly for Boston. From thence he sailed to Barbados, and thence to Charlestown, South Carolina. At all these places and at sea he made dip observations with both methods. A dip of 68° 22′ is given for Boston, and of $44\frac{1}{2}^\circ$ for Barbados, on p. 92. *These two dips precede by fifty-eight years any dip that has hitherto become known in the United States.* The vibration-times are unfortunately not given. A third dip instrument was sent with Captain Tempest to Antigua and St. Christopher's, a fourth sent with Captain Michel to Hamburg. The results with the last two instruments had not yet been all received at the writing of the book. Whiston does not give the actual observations, but says, on p. 90, "The original Journals are all in the Hands of my great Friend and patron, Samuel Molyneux, Esq., Secretary to his Royal Highness the Prince of Wales, and Fellow of the Royal Society: which Journals, when I have compleated the rest of the Observations I hope to procure, I intend to publish entire, for the more full Satisfaction of the curious." It seems that Whiston never published these records. It is hoped that the present article will induce some one to look them up. They may possibly be a valuable find.

In conclusion, let us sum up Whiston's achievements.

(1) Whiston drew the first isoclinics (1719-20).

(2) He invented the vibration method of determining the dip.

(3) He made the first relative terrestrial magnetic intensity observations (1720).

(4) The first intensity observations (1722), revealing the law of decrease of horizontal terrestrial magnetic force with approach towards magnetic pole, were made under his instructions.

L. A. BAUER.

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[11] The only thing that Whiston says with respect to the method employed, is the following Passage on page 112, viz.: "The Difference of this *Strength* of the Magnetick Power, from its *Direction*, is most visible in my Second Map hereto prefix'd. Where I have all along set down the 'Seconds' wherein my Needle perform'd a single horizontal Vibration, at about 120 Degrees from the Magnetick Meridian, in most Places, whose Squares, when Allowance has been made for the different Obliquity of the several Directions as to our Horizon, will give us the different *Strength* of that Magnetick Power at those several Places; as does the angle of dip give us the different *Direction* of the same power there. Now, at first Sight, the former there appears to be irregular, and the latter regular; as is the Case also of our Terella" Why Whiston should have observed the vibration time 120° *from the magnetic meridian*, instead of across the magnetic meridian, the writer has not been able to ascertain. Whiston does not appear to have made any further use of his observations.

[12] "The Calculation of Solar Eclipses without Parallaxes . . . . with an Account of some late Observations made with Dipping-Needles, in order to discover the Longitude and Latitude at Sea." (London, 1724.)

THE TEACHING UNIVERSITY FOR LONDON.

DURING the last week very satisfactory progress has been made towards the reorganisation of the University of London as a teaching as well as an examining body. In the first place, King's College has been brought into line with the other teaching institutions of the metropolis by expressing a general assent to the recommendations of the Gresham Commission, coupled with the proviso that any Statutory Commission appointed to give effect to the Gresham Commissioners' recommendations should have power to make such modifications in the scheme as may seem to them expedient after consultation with the bodies affected—a proviso already insisted on by every teaching institution that has expressed its general approval of the scheme.

The adhesion of King's College to the views of the other teaching institutions mentioned in the Report of the Gresham Commission, was made known on the eve of the reception by Lord Rosebery of the deputation of delegates from the London colleges, and made it possible for these to present their case with the strength derived from complete accord.

On Tuesday last, Lord Rosebery received two deputations—one in the morning in favour of the Gresham scheme, in which representatives of the Senate, the